

UNIVERSITÉ DE PARIS

FACULTÉ DES SCIENCES

LABORATOIRE DE GÉNÉTIQUE

13, RUE PIERRE CURIE, PARIS V^e

Tél. : ODÉon 16-40

September 3, 1953

Dear Joshua,

We have just returned from the Genetics Congress at Bellagio, where your absence was felt on a number of occasions. We would have welcomed a chance to talk with you and find out in more detail what you are busy with these days. Your letter last winter gave us a vague notion, and we hear indirectly, of course, from various itinerant scientists, but nothing can supplant discussion. A few people voiced the opinion that you are desirous of avoiding contact with microbial geneticists - an opinion which I find gratuitous. After all, anyone should be able to abstain from a congress without prodding speculation, especially since there are so many ^{meetings} and they are really often more tiring than profitable.

I presume the speculations have arisen because everyone is aware that an interpretation of sexual recombination in *E. coli* has been proposed which differs from your own. Certain people may be desirous of a confrontation of opinions, of which

yours would necessarily be the most authoritative.
I am inclined to believe that such a confrontation would not be especially profitable, and, in particular, because of a lack of decisive experiments to eliminate one or another interpretation, that such a confrontation would be apt to be more partisan than scientific. Consequently, if there is any basis to the contention that you are abstaining from meetings to avoid "battle", it has my entire sympathy and comprehension. Doubtless these rumors filter back to you, and I hope that you understand that they arise only because of the importance of your contribution to genetics. It is because we have very genuine admiration of the work of your laboratory, and feelings of friendliness toward you and Esther, that I feel like trying to establish a contact with you which will be purged of a certain murkiness which rumors inevitably create. You are very much talked about, you know, because you have made such a strong contribution. But it really doesn't change the fact that the people who feel friendly toward you remain that way, and those who don't, stay malicious.

Of course, I have a second reason for writing you — bad conscience. It is possible that the "note on terminology" in nature may have made you feel that the people who wrote it are hostile to you. I should like to reassure you

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that this is not at all the case.
It was a single outburst of humor
at the trend in all of us to create
new terms for our post-processes.

You are certainly no more culpable in this respect than
anyone. It just happened that the joke could crystallize a
little more readily. Having crystallized, it then became
tempting to see if a serious journal, such as "Nature", would
slip so badly as to publish it. I must admit that when
it came out, it had lost all its savor for us, and we
rather regretted it, fearing that it might make you feel
more isolated from us. I do hope you will accept our
apologies, laugh with us, and treasure an autographed
copy.

As for work, both Boris and I have had a very active
year. In my own case, I undertook ^{the investigation of} several problems,
and only one is in a more or less terminal phase. With Lohreit,
and Watson I worked out the x ray sensitivity of Collier's Strepto-
mycin resistance TP, (it being capable of quantitative treatment),
and found the subject complicated but interesting. In a word,
it looks as though TP has the sensitive volume about equal
to ^{that of} a small phage — which is what the very good papers of
Hulse, Crew, and Pollard claimed. I feel, therefore, that it is likely
that we shall find linkage groups in TP's, and that my
allozymic transformation can be pictured as some kind of cross over
between pseudoparallels — I do not believe, however, that TP's are

phages, but hope to have some very clear experiments on this point before very long. This coming year may bring very major advances in the chemistry of T's, and on their physical properties.

Boris, working with Roman, and Helene Bottriguer, have discovered that "petites" are of two sorts: "neutral" and "suppressive". As luck would have it, all the early work was with neutrals, for otherwise they would never have unravelled the story. Neutrals, in a cross with normal "grandes", behave in an entirely passive way: cytochrome synthesis is restored in the diploid, essentially at the moment of copulation, and all progeny are normal. Suppressives, on the contrary, when crossed with "grandes" suppress cytochrome synthesis in almost 100% of the diploids arising from copulations. All spore progeny from these "suppressed" diploids are mutant. The suppressive character arises simultaneously with the mutation of grande to petite, and there is thus a real possibility that they are dealing with a competitive cytoplasmic particle. On the other hand, there is an at least superficial resemblance between this change, and the conversion of pro-phage into phage. It is going to be most interesting to see where they go from here in the work.

I must get to work now - Boris joins me in sending you ^{both} our very kindest regards -

I entirely subscribe to the feelings expressed in this letter, and send both of you my regards. Boris

Sincerely,

Herriott.